The Sources of Wage Variation and the Direction of Assortative Matching: Evidence from a Three-Way High-Dimensional Fixed Effects Regression Model

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Abstract

This paper estimates a wage equation with three high-dimensional fixed effects, using a longitudinal matched employer-employee dataset covering virtually all Portuguese private sector wage earners over a 26-year interval. First, the variation in log real hourly wages is decomposed into three components reflecting worker, firm, and job title characteristics and a residual element. It is found that worker permanent heterogeneity is the most important source of wage variation accounting for one third of the wage variance, while firm permanent effects contribute one fourth. Job title fixed effects still explain a considerable one fifth of wage variance. Second, having established that high-wage workers tend to match with high-paying firms, worker fixed effects from the wage equation are next correlated with firm fixed effects from sales and value-added production equations to provide unambiguous evidence on the sign and strength of assortative matching. The correlations are positive and large, indicating that higher

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productivity workers tend to match with higher productivity firms.

Keywords: *JEL classification*: J2, J41.

Keywords: high dimensional fixed effects, wage decomposition, assortative matching

1. Introduction

This paper seeks to provide a better understanding of the sources of wage variation and the role of sorting. Its contribution is twofold. First, in explaining wage variability, it disentangles the effects of demand-side determinants of wages from supply-side determinants in the manner of Abowd, Kramarz, and Margolis (1999) (AKM), while adding job title fixed effects to the mix of worker and firm fixed effects estimated by these authors. Job title effects reflect the distinct set of occupational tasks performed by workers that serve to define occupational boundaries. Second, it addresses the problem of non-monotonicity typically encountered when estimating the correlation between worker- and firm fixed effects (even if filtered from job title heterogeneity) obtained from wage equations because wages do not necessarily increase in firm productivity. It provides an alternative way of addressing the correlations between the contributions of worker and firm heterogeneity. It does so by observing the sales of firms – and, for a restricted set of the data, information on firm value added – to estimate production equations identifying firm fixed effects. That is to say, worker (wage) fixed effects are correlated with firm (productivity) fixed effects to identify the sign and strength of assortative matching. The study furthermore draws on a unique matched worker-firm panel that has the advantages of covering virtually all Portuguese workers over the interval 1986-2013.

To anticipate our findings, it is first reported that worker permanent heterogeneity is the most important source of wage variation (33 percent), followed by firm permanent effects (24.6 percent) and by job title effects (19 percent). Note that the latter effect well exceeds the contribution of the education variable in conventional earnings equations, although of course the allocation of workers to job titles will clearly be influenced by education levels. Also the worker fixed effects contribution is reduced materially in the three fixed effects specification, even if remains true that 'what workers are' remains more important than 'what workers do' and 'for whom.' The second and indeed major result is that the correlations between the firm fixed effects obtained from the production function and the worker fixed effects derived from the wage equation are positive in sign and large in magnitude, albeit declining over time, suggesting that higher productivity workers tend to match with higher productivity firms. Interestingly, the separate correlations between the worker (wage) fixed effect and the firm (wage) fixed effect are also positive – contrary to similar such correlations in the literature based on the AKM approach – and in common with it also declining over time.

The plan of the paper is as follows. Section 2 contains a discussion of the nature of worker, firm, and job title fixed effects, together with a statement of the evolving literature on the complementarity between individual and firm productivity levels (i.e. assortative matching). A bare bones review of Portuguese wage setting precedes a concise description of the data in Section 3. The general empirical framework necessary to estimate wage equations with worker, firm, and job title fixed effects is next reviewed in Section 4. Wage variability is decomposed into its various components in Section 5, where the determinants of worker, firm, and job title fixed effects are investigated and correlations between these components of compensation are addressed. Section 6 addresses wage sorting and in particular the direction of the correlation between worker and firm fixed effects by worker and firm characteristics, inter al., as well as its sensitivity to sampling plan (i.e. limited mobility bias). Section 7 considers assortative matching proper, now using wage and productivity data to assess the relationship between firms wage policies and the productivity of their labor forces and the robustness of the association. Section 8 concludes.

2. Wage Variation and Worker-Firm Complementarities

2.1. The sources of wage variation

An important research theme in labor economics is why similar workers receive different remuneration and why similar firms pay different wages (Diamond, 1982b). There are two lines of reasoning to explain observed wage variability, one of which relies on standard competitive market forces (workers' characteristics) and the other on demand-side factors (employers' characteristics).

In a labor market operating under perfect competition, each worker should receive a wage that equals his or her marginal (revenue) product. Wage differentials should reflect differences in worker productivity rather than depend on job or employer attributes (other than those affecting worker utility such as dangerous working conditions that will in normal circumstances attract a compensating differential). In turn, worker productivity has a basis in competence – whether observed or not – typically 'acquired' through investments in human capital. Here we are abstracting from issues of unobserved intrinsic ability (Griliches, 1977) and associated signaling models (Spence, 1973).

There is no shortage of models seeking additional or alternative explanations for wage variability, but it is now the characteristics of firms rather than those of workers (i.e. worker competence or worker productivity differences) that assume pole position. Given the plethora of such treatments (e.g., implicit contract theory, principal-agent models, and efficiency wages), we will consider just two models that pose perhaps the sharpest contrast with the standard competitive model. The first approach has a basis in rent-sharing/insider-outsider considerations, while the second emphasizes labor market frictions.

Rent-sharing models predict that wages depend on the employer's ability to pay. In particular, wages are predicted to have a positive correlation with firm profits, since firms may find it beneficial to share their gains with their workers and pay above the going rate.[1] These models explain why wages depend not only on external labor market conditions but also on firm productivity, profits, degree of competition, and turnover costs, as well as the bargaining strength of workers. They also explain why the wages of workers from different groups of occupations, educational categories, and seniority tiers are higher in some firms or industries than in others.

The other explanation for wage differentials among workers with similar characteristics targeted here derives from the job search and matching literature and emphasizes the role of labor market frictions in wage determination. Thus, the equilibrium job search model of Burdett and Mortensen (1998) predicts that firms may have incentives to offer higher wages than their competitors in order to guarantee a low quit rate and attract a large number of workers in a market characterized by the existence of frictions – even in circumstances of homogeneous workers and firms *ex-ante*. This model predicts that wages are increasing in firm size and workers' job seniority. For their part, matching models that also take into account the existence of frictions in the labor market provide an explanation for wage dispersion. In the models of Diamond (1982a) and Mortensen (1982), for example, while the wage is set by the employer, workers and firms bargain over the share of the matching rent *ex post*. Differences in match productivity, then, explain why similar workers (firms) may receive (offer) different wages.[2]

Our goal in the present exercise is to disentangle the effects of employers' decisions (demand-side determinants of wages) from the effects of choices made by workers (supply-side determinants) in the explanation of wage variability. To this end, researchers have estimated wage regressions incorporating both worker

[1]The earliest rent-sharing studies using industry data (e.g. Dickens and Katz, 1987) were followed by firm studies (e.g. Hildreth and Oswald, 1997; Arai, 2003). Modern treatments use matched employer-employee data to control for unobserved worker abilities (e.g. Guertzgen, 2009; Card et al., 2013).

[2]For treatments combining equilibrium job search *and* matching, see Quercioli (1998); Robin and Roux (2002); Mortensen (2000); Rosholm and Svarer (2004); Cahuc et al. (2006).

and firm fixed effects, beginning with the additive worker and firm effects model proposed by AKM (1999) in their pioneering work using a French longitudinal matched employer-employee dataset. However, besides worker and firm heterogeneity, a third important dimension of wage formation is *job title heterogeneity*, reflecting the distinct tasks performed by workers that define the set of occupational boundaries. There are a variety of reasons why job title heterogeneity can be expected to influence wage rates. One is compensating advantages for riskier and/or less pleasant working environments. Another is provided by the heavy doses of job specific training that some jobs may entail. Additional reasons include occupational crowding and collective bargaining (encompassing different wage floors, promotion policies, and negotiated job titles). A high job title fixed effect is a job title remunerated at higher than expected levels after allowing for the observed and unobserved permanent characteristics of firms and workers. To properly incorporate these determinants of wages one needs a very detailed accounting of the kind of jobs being undertaken by workers. Even a highly disaggregated occupational count is unlikely to be fit for purpose here because an employer's wage policy for the same occupation (e.g. a secretary) might be governed by different collective agreements (say the banking industry collective agreement as opposed to that for the retail trade sector) or, in the case of a single industry agreement, individuals ostensibly doing the same job within a firm might receive different wages and hence occupy different job titles given their different exposure to risk.

Fortunately, the dataset used in the present inquiry contains an unusually rich set of information enabling us to identify the collective agreement that regulates the employment contract applicable to each worker. Moreover, within each collective agreement, we can further pinpoint the exact, detailed occupational category of each worker. Each year, around 300 different collective agreements are negotiated in Portugal (see below) that define wage floors for each particular job title (so-called *categoria profissional*). On average, each collective agreement defines the wage floor for around 100 job titles. Overall in a given year, there are 30,000 collective agreement/job title combinations to which workers can be allocated. The main use of the dataset – the *Quadros de Pessoal* and its successor survey the *Relatório Único* (see below) – is precisely to enable the officials of the Portuguese Ministry of Employment to ascertain whether employers are in compliance with what was actually agreed to at the bargaining table (i.e. wages, work schedules, and other conditions). This recording obligation also serves to underscore the accuracy of the Portuguese data.

By properly taking job title effects into account one should be able to provide refined estimates of worker and firm fixed effects, and shed additional light on the current debate concerning the role of assortative matching. In the process, we should also be able to disentangle the joint contribution of collective contracts and occupational category to wage formation, using dummies for each contract and each occupation.

The objective of this part of the estimation, then, is to calculate the contribution of worker, firm, and job title fixed effects to overall wage variability. The requirements of this decomposition exercise are daunting; specifically, the availability of longitudinal datasets combining information on firms and their employees (namely, matched employer-employee datasets with unique identifiers for firms, workers, and job titles) and the use of appropriate panel data econometric techniques to estimate three high-dimension fixed effects in wage equations. Fortunately, panel datasets have become available in recent years for many countries, while econometric tools (and computing capacity) have also improved greatly. Taken in conjunction, all three ingredients – data, econometric techniques, and computing facilities – have made it possible to bring new information to bear in the empirical debate on (many aspects of) wage determinants. As was noted earlier, AKM were the first to propose an empirical framework for estimating worker and firm effects in wage equations. They reported that worker characteristics explained the major part of wage differentials, of inter-industry wage differentials, and of firm-size wage differentials.

Here, we shall use a longitudinal matched employer-employee dataset covering virtually all employees in Portugal (see below). In estimating a wage equation that includes worker and firm fixed effects, we use a routine that was especially developed in *Stata* providing an exact solution to the least squares problem that arises when dealing with very high dimension matrices. However, we take this methodology a stage further by adding a third fixed effect in our wage equation so as to control explicitly for job title heterogeneity.

2.2. Assortative matching

The sorting of heterogeneous workers across firms has been the subject of not inconsiderable debate. The idea behind *positive* assortative matching is the complementarity between individual and plant productivity levels, with good workers being teamed up with good firms. The theoretical basis for such matching is provided by assignment models. In his marriage market model, Becker (1973, 826) shows that if the production function is supermodular the unique equilibrium that occurs is both efficient and characterized by perfect sorting. The early assignment models, however, were rooted in competitive equilibrium (e.g. Sattinger, 1993; Kremer and Maskin, 1996), thereby disregarding establishment-specific components in the wage equation. With the introduction of frictions, more recent developments have ensured a sorting of workers across plants (Shimer and Smith, 2000; Shimer, 2005; Postel-Vinay and Robin, 2002). At issue in these models is the nature of the equilibrium matching pattern since different matching models predict different patterns (i.e. admitting of either positive or zero/negative assortative matching) according to the assumptions of the model.

Empirical work has often failed to produce evidence of positive assortative matching in the wake of AKM's (1999) pioneering study. Using matched employer-employee data for 1976-1987 for a 1/25th sample of the French labor force, these authors decomposed wages into fixed firm and person effects and reported a positive albeit weak correlation between the two. However, these results were obtained on the basis of statistical approximations, limited by the capacity of the computers on which they were generated. In re-estimating the model using exact methods, Abowd et al. (2002) report that the correlation between the person and firm effect is -0.283 (rather than 0.097 using the former methodology). The authors also report correlations between the two effects for a 1/10th sample of employees in the state of Washington, using matched data for 1984-1993. The corresponding coefficients were -0.025 and 0.050 for the exact and approximate estimates, respectively. And, as noted earlier, negative correlations have indeed figured largely in the literature using the wage data approach (e.g. Goux and Maurin, 1999; Gruetter and Lalive, 2009), although a notable exception is Card et al. (2013) in an investigation of German earnings dispersion over 1985-2009.

Although negative assortative matching may have an economic explanation (see also Woodcock, 2010), research has sought to determine whether this result might be an artifact of the use of standard econometric techniques. Abowd et al. (2004) test and discount the notion that the negative correlation between the fixed worker and employer effects (i.e. good workers gravitating to bad firms) is caused by limited mobility bias in the estimation of each effect. They conclude that while sampling error does impart downward bias to the two effects, its magnitude is simply too small to modify the basic negative result for France or the absence of correlation for the United States (i.e. random assignment). A more forthright conclusion is reached by Andrews et al. (2008), who show that the correlations between the two fixed effects will be downwardly biased if there is true positive assortative matching and when any conditioning covariates are uncorrelated with the two fixed effects. The authors' simulations indicate that the extent of bias is a decreasing function of worker mobility which in turn reflects the propensity to move, the length of the panel, and the average size of firms. In applying formulae to correct the bias to West German matched employer employee data for 1993-1997, the authors find evidence of not inconsiderable bias: some 25 percent for the full sample, increasing to around 50 percent for the subsample of movers. Although in this study the biases are large, they do not overturn the negative correlation between the worker and plant effects. However, in their subsequent analysis of social security records for three German *Länder*, Andrews et al. (2012) report that low mobility bias does indeed obscure an estimated correlation that is strongly positive.

A recent paper by Card et al. (2018) reviews the various technical reservations that attach to the AKM model – including, in addition to limited mobility bias, the identification of age and time effects, exogenous mobility, and additive separability – in a framework that sees that model alongside studies of rent seeking as offering key insights into understanding the determinants of wage inequality. The findings of both literatures are then interpreted using a search model, or more strictly a monopsony model in which workers' idiosyncratic tastes for different workplaces allow firms to set wages unilaterally and share rents only by reason of information asymmetries. That is, a microeconomic model is used to mimic the results obtained from rent seeking studies and the AKM two-way fixed effects estimator, in both of which settings workers bargain with firms over wages. In the process, Card et al. (2018) necessarily offer, in addition to findings from cross-sectional and within-job models of rent sharing, results on the relationship between the AKM components of wages and mean log value added per worker. This study uses matched employer-employee data for Portugal, 2005-2009. As the base dataset partially coincides with that used in the present inquiry, we will have occasion to further comment on the results obtained by Card et al. (2018) in section 7.

The perception that one cannot distinguish positive from negative sorting using wage data – or the related concern that theoretical models can generate positive or negative correlations between firm and person effects from a wage equation – explains why some have advocated using a *productivity model* directly rather than inferentially. Unlike the more numerous studies employing wage data, those using output data point decisively to positive assortative matching. As a case in point, using Portuguese matched employer-employee data from the *Quadros de Pessoal*, 1986-2000, and a translog specification, Mendes et al. (2010) estimate a firm-specific productivity effect for each firm that they then relate to the skills of workers in the firm measured as the time average of the share of highly-educated workers in the firm. They find evidence of positive assortative matching, especially among longer-lived firms. They report that the results are not caused by heterogeneity in search frictions by worker skill type.

Other studies have centered squarely on issues of bias in the standard model. Thus, Lopes de Melo (2008) argues that the approach to measure sorting using worker and firm fixed effects in a log-linear wage regression as proxies for worker constant heterogeneity is biased against detecting it. His sorting model with frictions yields strong positive sorting, with good workers teamed with good firms because of complementarities in production. It is argued that this outcome is hidden because of non-monotonicities in the wage equation caused by the interaction between wage bargaining and the limited ability of the firms to post new vacancies. This in turn arises because high productivity firms have better outside options than their low productivity counterparts, which causes downward pressure on the wages of their workers, especially low-wage workers who are then paid less when working for a more productive firm. Lopes de Melo's distinct solution is to examine the correlation between a worker's wage fixed effect and the average fixed effect of the coworkers in the same firm. His correction yields strong evidence of positive assortative matching, unlike the conventional measure which yields an absence of sorting when applied to Brazilian matched employer-employee data, 1995-2005. In a subsequent analysis, Lopes de Melo (2018) demonstrates that his earlier results on sorting were not driven by worker composition. More importantly, he uses the AKM correlations as moments to match in a structural model. The model performs well in explaining the correlations between fixed effects if not the dispersion in firm fixed effects.

In the final (structural) study considered here, Hagedorn et al. (2017) eschew use of a fixed effects regression approach while nonetheless accepting that all parameters of the classic model of sorting based on absolute advantage (i.e. workers and firms can be ranked on their productivity) with search frictions can be identified using only matched employer-employee data on wages and labor market transitions.[3] Hagedorn et al. (2017) develop strategies for rank-

[3]Only Eeckhout and Kircher (2011) argue that the use of worker and firm fixed effects does not enable one to conclude anything about assortative matching as the particular non-

ing worker and firms, as well as an implementation algorithm. The ranking of workers in this very different schema proceeds on the basis of deriving several (equivalent) statistics that are monotonically increasing in worker type, x: the lowest and highest accepted wages in firms, and the adjusted (for unemployment) average wage. For its part, firm ranking proceeds on the derivation of a statistic that is monotonically increasing in firm type, y. The value of a job vacancy is first established to be monotonically increasing in a firm's productivity and, since the surplus of a vacancy is also increasing in y, Nash bargaining implies that the average surplus of workers is also increasing in y. The authors then show that this surplus can be expressed as a function of wages; specifically, the worker's surplus is proportional to the difference between the wage and the reservation wage. The latter statistic is increasing in firm type and is the basis for ranking firms. The link is that, once workers are ranked, similarly ranked workers must have similar reservation wages. Having ranked workers and firms, the rank correlation between workers of type x and firms of type y denotes the direction and strength of sorting. [4]

What is our takeaway from the foregoing? Taken in the round, the pessimistic assessment of the literature on the use of worker *and* firm fixed effects derived from the wage equation to address assortative matching should not be taken to rule out the use of worker fixed effects derived from the wage equation. Rather, the critique hinges on the particular derivation of the firm fixed effect given that firm productivity is not increasing in wages. Thus, Lopes de Melo (2008, 3) states: "We propose an alternative measure of sorting, still based on the same fixed effects methodology, that captures the degree of sorting in the

monotonic effect of firm type on wages translates into a wage that cannot then be decomposed into an additively separate worker and firm fixed effect. However, as shown by Hagedorn et al. (2017, 43 et seq.), the specific modeling choices made by these authors "ultimately prevent the identification of the model."

[4]The actual value of the match is recouped from a production function obtained by inverting the wage equation. The determinants are actual wages and the two measured outside options, namely the value of a vacancy and the value of unemployment.

model remarkably well: the correlation between a worker fixed effect and the average fixed effects of his coworkers." *Vulgo*: the conventional worker fixed effects are retained, only the firm-fixed effects derived from the wage equation are replaced. Lopes de Melo (2008, 4) reports that non-monotonicities do not affect the ordering of wages across workers, noting that wages (and average worker wages) are unambiguously increasing in worker skill (but not firm productivity), and that in simulations worker fixed effects almost perfectly capture the relative ranking of workers. Not even Eeckhout and Kircher (2011, 879-880) claim that wages $w(x, y)$ are non-monotone in x (only in y). Rather more positively, they confirm that the wage (unlike profits and output) is increasing in worker type (pp. 879-880) and alone permit identification of worker type (p. 900). Only Hagedorn et al. (2017, 30) critique the notion that wages are monotone in worker productivity, but nevertheless conclude that the "key problem ... underlying the fixed effect regression is the assumption that wages are monotone in firm's productivity (fixed effect)" because it is inconsistent with sorting models incorporating search frictions.

Still at issue is the use of the standard correlation between worker and firm fixed effects as a moment to match in structural models such as those of Hagedorn et al. (2017) and Lopes de Melo (2018). This is because the results that are obtained in these treatments may be sensitive to additional assumptions in the model (Borovičková and Shimer, 2017, 5-6). This caveat serves also to caution against the considerable appeal of exercises such as the former study that require data from matched employer-employee data on wages and labor market transitions alone in a model more firmly located in search theory.

In recognition of non-monotonicities in firm type noted in the evolving literature, we shall refine our treatment of assortative matching to include firm level productivity filtered from the heterogeneity of labor inputs. That is, we retain the worker fixed effect but replace the AKM firm effects with firm productivity estimates derived from a Cobb-Douglas production function where labor input is differentiated by job title. We will assume that endogenous mobility bias is not a cause of great concern, but show how limited mobility bias is less important in longer samples.

3. Institutional Context and Data

3.1. Wage Setting Framework

Wage setting in Portugal is dominated by the widespread use of government extensions of sectoral agreements and the presence of mandatory minimum wages. There is a modicum of firm-level bargaining but formally decentralized bargaining of this nature is the exception rather than the rule – covering less than 10 percent of the workforce – and often taking place in large enterprises that were formerly part of the public sector. Sectoral agreements predominate. They are conducted by employer and union confederations and may cover a wide range of industry-specific occupations. That said, the system does not rule out parallelism or overlapping collective agreements, such that a single enterprise may be covered by two or more agreements depending on the union affiliation of its workers. Indeed, the situation may be further stratified if the firm in question straddles more than one line of economic activity, thereby belonging to more than one employers' association. As a result of union fragmentation, therefore, several agreements may coexist for the same region, occupation, and firm. Horizontal agreements, covering a number of sectors, are also possible, but are not frequent. Overall, coverage of collective agreements in the Portuguese private sector is above 90 percent.

Collective bargaining in Portugal differs from that in other nations by virtue of its fragmentation and extent of multi-unionism. The corollary is that the contents of collective agreements are at once extensive and general. They are extensive insofar as they cover many categories of workers. They are general in that they set only minimum conditions of which the most important is base level monthly wages, although some agreements include normal working hours and overtime pay. The focus is upon wage floors – on average branch agreements have set wages for around 100 job titles, or *categorias profissionais* – rather than anticipated wage growth that in some centralized bargaining regimes (e.g.

Sweden) is directly incorporated into sectoral agreements. In consequence, employers have freedom of maneuver to tailor remuneration to their prevailing economic circumstances:[5] Collective bargaining sets wage levels not wage changes.

The most relevant mechanism shaping the formation of wages is the systematic extension of industry-wide agreements by the Ministry of Employment. Even though by law the collective agreement only binds the trade union members and the employer associations' affiliated firms that are parties to the agreement, not only are voluntary extensions common but also and altogether more importantly industry-wide agreements are systematically extended throughout the sector via so-called *portarias de extensão* by the Ministry of Employment following a request from either or both of the parties to the agreement. This means that even wage agreements reached by trade unions and employers associations with very low representation have a strong impact in setting wage floors. Indeed, in any given year, allowing for this near automatic extension procedure, collective bargaining determines around 30,000 minimum wages that correspond to 30,000 job titles.

Finally, wage floors are also set under national minimum wage machinery, established in 1974. The minimum wage can exceed that set under sectoral bargaining. In this event of course the former dominates. Currently, the national minimum wage covers some 16 percent of full-time wage earners. We will subsequently examine in section 6 how restricting the sample to minimum wage earners influences the correlation between worker and firm fixed effects.

3.2. Data

The Portuguese data used in this inquiry come from a longitudinal matched employer-employee dataset known as the Tables of Personnel (or *Quadros de Pessoal*) for the years 1986 to 2009 (excepting 1990 and 2001) and from its virtually identical successor survey the Single Report (or *Relatório Único*) for the

[5]On the determinants of the contractual wage and this 'wage cushion,' see Cardoso and Portugal (2005); Addison et al. (2017).

years 2010 to 2013. This unique dataset is administered by the Portuguese Ministry of Employment, and is taken from a mandatory annual survey of all firms with at least one wage earner in the reference month – March of each year until 1993, October thereafter. The survey covers various firm and establishment characteristics, as well as a set of characteristics of the workforce (see below). Being compulsory, it does not suffer from the non-response problems that often plague standard household and firm surveys. Further, the survey covers all Portuguese wage earners, with the exceptions of the Public Administration sector and domestic servants.

Turning to specifics, the dataset includes information on the establishment (identifier, location, industry, and employment), the firm (firm identifier, location, industry, legal form, ownership, year of formation, employment, sales, and capital), and its workers (social security identifier, gender, age, education, skills, occupation, employment status, professional level, seniority, earnings [base wage, seniority-related earnings, other regular and irregular benefits, and overtime pay], normal and overtime hours, time elapsed since last promotion, professional category and the corresponding classification in a collective agreement).

For the purposes of this exercise, a subset of variables was selected, certain new variables created, and some observations removed. The final set of variables retained for analysis is given in Table A.1. Among the restrictions placed on the data were the exclusion of those individuals who were not working full time, who were aged less than 16 years or more than 64 years, who earned a nominal wage less than 80 percent of the legal minimum wage, who recorded errors in their admission/birth dates, and who had duplicate social security codes or other errors in those codes. Individuals employed in the agriculture, hunting, forestry and fishing sectors – together with misclassified industries – were also excluded.

Further, we also excised close to 2 percent of observations that did not belong to the largest connected set. For a model with two fixed effects, Abowd et al. (2002) noted that for identification purposes one needs to impose one restriction on the coefficients for each *mobility group* in the data; where a mobility group contains all workers and firms that are connected, comprising all the workers who ever worked for any of the firms in the group and all the firms at which any of the workers were ever employed. With several mobility groups (and thus several restrictions) the estimated coefficients of the fixed effects are not comparable across groups. If these coefficients are of interest, then a simple solution is to work only with the largest mobility group which usually comprises the majority of the observations. With three fixed effects a similar logic applies. Since we want to use the estimates of the fixed effects for posterior analysis, we restricted the data set to connected observations for which comparability of the estimates of the fixed effects is assured. [6] As noted above, the largest group accounted for more than 98 percent of the data set.

Our final dataset for all 26 available survey years comprises 36,558,896 observations drawn from 649,589 different firms, 5,945,393 individual workers, and 133,598 job titles (i.e. the code of the variable that results from the conflation of the professional category variable and the corresponding collective agreement variable).

We also make use of the *Central de Balanços* (CB) dataset, which contains yearly economic and financial information for all non-financial Portuguese corporations since 2006. The available information allows us to compute an accurate measure of firm value added which we then merge into our final dataset. Matching of the two datasets still leaves us with 253,651 firms.

4. The General Empirical Framework to Decompose Wage Variation

Consider the problem of estimating a standard Mincerian wage equation to which we add three high-dimensional fixed effects to account for firm, worker, and job-title heterogeneity:

$$\ln w_{ifjt} = \mathbf{X}_{ift}\beta + \theta_i + \phi_f + \lambda_j + \varepsilon_{ifjt} \ . \tag{1}$$

[6] The algorithm of Weeks and Williams (1964) was used to identify connected observations. When this algorithm is applied to the setting with two fixed effects we obtain the *mobility groups*.

In the above equation, $\ln w_{ifjt}$ stands for the natural logarithm of the real hourly wage of individual i $(i = 1, ..., N)$ working at firm f $(f = 1, ..., F)$ and holding a job title j $(j = 1, ..., J)$ at year t $(t = 1, ..., T_i)$, while $\mathbf{X}_{ift}$ is a vector of k observed (measured) time-varying exogenous characteristics of individual i and firm f. There are T_i observations for each individual i and a total of N^* observations. All time-invariant characteristics of the workers, firms and job titles are captured by the fixed effects which are, respectively, θ_i, ϕ_f, and λ_j. We assume that exogenous mobility holds in our model, meaning that conditional on the explanatory variables, the random term ε_{ifjt} has an expected value of zero. [7] According to this equation, there are five distinct sources of wage variability:

1. the observed time-varying characteristics of workers, firms, and the economy $(\mathbf{X}_{ift}\beta)$;
2. non-time-varying worker heterogeneity (θ_i);
3. non-time-varying firm heterogeneity (ϕ_f);
4. non-time-varying job title heterogeneity (λ_j); and,
5. unexplained random variation (ε_{ifjt}).

Equation (1) includes three high-dimensional fixed effects. Estimation of linear regression models with more than one high-dimensional fixed effect poses some particular challenges. The problem was first tackled by Abowd and Kramarz (1999) and Abowd et al. (1999). In their seminal papers, these authors proposed a computationally tractable solution that yielded an approximation to the full least squares solution of a linear regression model with two high-dimensional fixed effects. In a later paper, Abowd et al. (2002) presented a conjugate gradient algorithm that led to the exact least squares solution of this problem. More recently, Guimarães and Portugal (2010) demonstrated that it is possible to

[7] A recent study by Card et al. (2016) has shown that this assumption is reasonable for the Portuguese data. The authors applied the tests for exogenous mobility proposed by Card et al. (2013) to the *Quadros de Pessoal* dataset. They did not find any evidence supporting the correlation of the residual components of wages with specific patterns of mobility.

obtain the exact least squares solution for linear regression models with two or more high-dimensional fixed effects with a full Gauss-Seidel iterative algorithm.

5. The Role of Individual, Firm, and Job Title Heterogeneity in Wage Differentials

In order to decompose wage variability into the components identified earlier, we first estimated equation (1). The explanatory variables (or observed time-varying characteristics) are age squared,[8] tenure, tenure squared, firm size, and 23 (survey) year dummies. The dependent variable is the natural logarithm of the real hourly wage. The results are reported in Table 1, with separate results being provided in the first column of the table for the two high dimensional fixed effects case. In both columns, as expected, wages increase with tenure and, familiarly, larger firms also pay higher wages. Observe that the R^2 terms of both equations are considerably higher than observed for standard wage regressions. Thus, the worker fixed effects, firm fixed effects, job title fixed effects, and worker and firm time-varying characteristics together explain 89.2 percent of the variability in real wages. That said, the addition of job title fixed effects does not add materially to the explanation of wages, as without them the R^2 is still 88 percent. Rather, as we shall see, the contribution of job title fixed effects accrues via its role as a component of wage variation (see Table 5, below).

Table 1 around here

In this framework, it will be recalled that the worker fixed effects (θ_i) include both the workers' unobserved and observed but non-time-varying characteristics. Similarly, the firm fixed effects (ϕ_f) and job title fixed effects (λ_j) include both the unobserved and observed but non-time-varying firm and job title characteristics, respectively. We decomposed the three estimated fixed effects into

[8] A linear term in age is excluded as its coefficient cannot be estimated in a model that includes worker fixed effects and year fixed effects. This is simply a manifestation of the conventional cohort-age-calendar year identification problem.

these components by estimating the following three regression equations: first,

$$\widehat{\theta}_i = const. + \mathbf{W}_i\eta + \varepsilon_i \ , \tag{2}$$

where $\mathbf{W}_i$ is a vector of non-time-varying worker characteristics (comprising gender, education, and birth-year dummies), η is the associated vector of coefficients, and $\mathbf{W}_i\eta$ is the worker non-time-varying observed characteristics component. Note that α_i, the worker specific intercept α_i – capturing the effect of worker unobserved characteristics, which can be interpreted as the opportunity cost or the market valuation of worker heterogeneity – is obtained residually by $\widehat{\alpha}_i = \widehat{\theta}_i - \mathbf{W}_i\widehat{\eta}$;

$$\widehat{\phi}_f = const. + \mathbf{Z}_f\gamma + \varepsilon_f \ , \tag{3}$$

where $\mathbf{Z}_f$ is a vector of non-time-varying firm characteristics (five regional dummies; capital ownership, specifically, the shares of domestic and public capital; and twenty-eight industry dummies), γ is the associated vector of coefficients, and $\mathbf{Z}_f\gamma$ is the firm non-time-varying observed characteristics component.[9] As before, the firm-specific intercept, φ_f, capturing the firm unobserved characteristics effect, is obtained residually, by $\widehat{\varphi}_f = \widehat{\phi}_f - \mathbf{Z}_f\widehat{\gamma}$; and third,

$$\widehat{\lambda}_j = FE_{occup} + FE_{ca} + \varepsilon_j \ , \tag{4}$$

where the sum of the two fixed effects (FE_j), one for the occupation variable FE_{occup} and the other for the collective agreement variable FE_{ca}, corresponds to the non-time-varying observed characteristics component and $\widehat{\delta}_j$, the job title specific intercept capturing the job title unobserved characteristics effect, is

[9]We assume that the variables included in $\mathbf{Z}$ capture the structural characteristics of firms, changes in which over time are either nonexistent or too small to be considered time-varying and requiring their direct incorporation as explanatory variables into equation (1). The same reasoning applies to the education variable for workers in equation (2), and to the occupation and collective agreement arguments in equation (4). Note further that the Portuguese industrial classification system changed in 1995. Because of this change, and given that the regression covers the entire period, we constructed an aggregated common classification comprising 29 different industries.

obtained residually by $\widehat{\delta}_j = \widehat{\lambda}_j - \widehat{FE_j}$. We now have the following compensation components (plus the residual):

- $\mathbf{X}_{ijt}\widehat{\beta}$: observed worker, firm, and economy time-varying characteristics that comprise three components: time dummies, time-varying characteristics of workers, and time-varying characteristics of firms.
- $\widehat{\theta}_i$: worker fixed effects.
  - $\mathbf{W}_i\widehat{\eta}$: observed worker non-time-varying characteristics.
  - $\widehat{\alpha}_i$: unobserved constant worker characteristics.
- $\widehat{\phi}_f$: firm fixed effects.
  - $\mathbf{Z}_f\widehat{\gamma}$: observed firm non-time-varying characteristics.
  - $\widehat{\varphi}_f$: unobserved constant firm characteristics.
- $\widehat{\lambda}_j$: job title fixed effects.
  - $\widehat{FE_j}$: observed job title non-time-varying characteristics.
  - $\widehat{\delta}_j$: unobserved constant job title characteristics.

Tables 2 and 3 report the estimation results for the worker fixed effects and the firm fixed effects regressions, respectively. Beginning with Table 2, we observe that the worker fixed effects for females are on average 14.3 log points smaller than those for men. Further, there is a monotonically increasing premium associated with the education level: a worker who has completed the second stage of tertiary education shows a fixed effect that is on average 54.3 log points larger than that of a worker with pre-primary or no formal completed education (the reference category). Note that these results are pure effects; that is, they result from a regression in which the dependent variable (the worker fixed effects) was estimated through a regression that controlled simultaneously for the time-varying characteristics of workers and firms and for firm and job-title heterogeneity. Overall, these non-time-varying worker characteristics explain 64.4 percent of the variability in worker fixed effects.

Table 2 around here

From Table 3 we see that the geographic location of the firm, together with its capital ownership, size (as measured by the number of employees), and industry affiliation play important roles in explaining the differences in the firm fixed effects. Specifically, the firm fixed effects are larger throughout all NUTS II (Nomenclature of Territorial Units for Statistics) regions than in the north region (the reference category) but especially so for Lisboa and the Algarve; the firm fixed effects tend to be higher among firms with larger shares of foreign capital; and there is also strong evidence of material differences in firm fixed effects across different industries. Note again that these effects are pure effects, as they result from a regression in which the dependent variable (the firm fixed effect) was estimated via a regression that controlled simultaneously for time-varying characteristics of workers and firms and for worker and job-title heterogeneity.

Table 3 around here

The estimation results for the job title fixed effects regression are not reported here as the explanatory variables are two high-dimension fixed effects. Note that equation (4) has a different specification from equations (2) and (3) above. This is due to the nature of the explanatory variables chosen for equation (4). Collective agreement and (harmonized) occupation are both categorical variables with too many outcomes to be included explicitly as dummy variables (1,516 and 447 different outcomes, respectively, for the entire period). Therefore, we decided to include them as two fixed effects. This is equivalent to the least square dummy variable approach (LSDV) of a fixed effects estimation. We can summarize the estimation results as follows: the R^2 of this equation is 0.696, meaning that the two observed non-time-varying job title characteristics (occupation and collective agreement) explain 69.6 percent of the variability in job title fixed effects. The largest role is attributable to occupation, as the R^2 of an equation containing only this variable explains 56.8 percent of the variability in job title fixed effects, whereas the R^2 of an equation with just the collective agreement argument explains 39.4 percent of that variability.

Table 4 around here

In Table 4, we report the correlations among the components of log real hourly wages. Of the four main components – time-varying characteristics, worker fixed effects, firm fixed effects, and job title fixed effects – the job title fixed effects component shows the highest correlation with log real total compensation (0.67), followed next by the firm fixed effects component (0.61), then by the worker fixed effects component (0.54), and finally by the individual and firm time varying characteristics component (0.23). Both the observed and unobserved components of the worker fixed effects are highly correlated with the log of real total compensation (0.35 and 0.43, respectively). Concerning the components of the firm fixed effects, the observable part is that most highly correlated with log real total compensation (0.53). The unobserved part of the firm component is less important in determining total compensation. As regards the components of the job title fixed effect, the observable part is also the most highly correlated with the log of real total compensation (0.62), while the unobserved part is altogether less relevant. In sum, with the exception of the worker fixed effect, the observable part of each component is more highly correlated with the log of real total compensation than the unobservable part.

In addition, we find that the correlation between firms' wage policies (as proxied by the firm fixed effects) and the quality of their workforces (captured by the worker fixed effects) is positive (0.20). Although not large, this value is nonetheless much larger than that reported in the literature. For example, Abowd et al. (2002) report a negative correlation for France and a correlation close to zero for the state of Washington (see also the lower estimates in Goux and Maurin (1999), using Labor Force Survey data).

The correlations in Table 4 also suggest an interpretation as regards sorting. In terms of observable characteristics, there is evidence of good workers tending to be found in high-paying firms: the correlation coefficient between the corresponding components of the firm and worker fixed effects is 0.26. These results are, then, partly consistent with this literature. However, as discussed earlier, we should resist the temptation of interpreting this positive correlation as evidence of complementarity between worker and firm levels of productivity.

Table 4 further indicates that the correlation coefficient between worker fixed effects and job title fixed effects (0.41) is larger than that between firm fixed effects and job title fixed effects (0.26). The latter effect indicates that high paying jobs tend to go hand in hand with high-paying firms. In both cases, the correlations are larger in terms of the observable characteristics of workers and firms (0.37 and 0.33, respectively).

Finally, these results are largely mirrored in Table A.2 which presents the correlations between compensation components for the two dimensional fixed effects specification. Thus, for example, we observe a positive correlation between firm and worker fixed effects of 0.23, closely similar to that reported earlier for the full model. In sum, these two sets of results indicate that the relationship between firms' wage policies and the quality of the workers they select is positive, while suggesting that there are certainly factors other than wage policies that explain the distribution of high-ability workers across firms.

Next, to measure the contributions of worker, firm, and job title characteristics (both observed and unobserved) to wage variation, we used the following decomposition:

$$\ln w_{ifjt} = \mathbf{X}_{ift}\beta + \alpha_i + \mathbf{W}_i\eta + \varphi_f + \mathbf{Z}_f\gamma + \delta_j + FE_j + \varepsilon_{ifjt} = \sum_{p=1}^{10} C^p_{ifjt} \;, \quad (5)$$

where the C^p_{ifjt} represent the individual summands ($\mathbf{X}_{ift}$ comprising the three components described above, namely time and the time-varying characteristics of workers and firms) of the wage equation. The contribution of each component, C^p_{ifjt}, can be calculated as:

$$\frac{cov(\ln w_{ifjt}, C^p_{ifjt})}{Var(\ln w_{ifjt})} \;, \quad (6)$$

where, by definition, $\sum_{p=1}^{10} Cov(\ln w_{ifjt}, C^p_{ifjt})/Var(\ln w_{ifjt}) = 1$.

Table 5 around here

In Table 5 we report the contribution of each component to real hourly wage variation. Separate results are provided for samples with two and three high dimensional fixed effects. Beginning with the latter, it can be seen that the largest contribution to wage variation comes from worker fixed effects (33.0 percent), followed by firm fixed effects (24.6 percent), job title effects (19.0 percent), and then by individual, firm, and aggregate time-varying effects (12.4 percent). There is a residual contribution of 11.0 percent. Accordingly, when comparing worker and job title effects, for example, it follows that what workers 'are' is more important than what workers 'do.' Nevertheless, the latter broadly matches the magnitude of the 'for whom' contribution. Of the worker fixed effects, the unobserved sub-component makes a marginally smaller contribution (16.4 percent) than do the gender, education, and birth-year sub-components (16.6 percent). For the firm fixed effects, the two sub-components contributions are again almost the same (at 12.6 percent and 12.0 percent for the unobserved and observed components, respectively). Finally, in the case of the job title fixed effects, the unobserved component makes a much smaller contribution (4.1 percent) than does the observed component (14.9 percent).

The contrast with the specification containing only two high dimensional effects is immediate, where 47.9 percent of wage variation is accounted for by worker fixed effects and 27.1 percent by firm fixed effects. Clearly, given that the firm fixed effects are close and the individual, firm, and aggregate time-varying effects practically the same in the two specifications, worker fixed effect component is picking up much of the job title effect in the forms of occupational and contractual heterogeneity.

6. A Detour on Wage Sorting

In this brief section we tackle two issues that have arisen in exploiting a log wage regression to estimate worker and firm fixed effects, more specifically their correlation. We shall refer to the correlation as providing evidence of wage sorting, as opposed to productivity sorting which cannot be identified directly from wage data alone. The first issue concerns the sensitivity of the correlation between worker and firm fixed effects to worker, firm, regional, and industry characteristics. The second issue, which has been more widely examined in the literature, is the tendency for the size and sign of the correlations between the worker and firm fixed affects to be biased downward the fewer the workers who move between firms in the data (see Andrews et al., 2012).

Table 6 around here

Table 6 presents the correlations between worker and firm fixed effects from the wage equation by age group, gender, tenure, education, broad wage category, firm size and age, region and industry. It is clear that the correlations are increasing in age, tenure, and education (up to the tertiary level at least). On the other hand, they are lower for college graduates and minimum wage earners. For its part, wage sorting is more pronounced for males than for females. Increasing wage sorting by age and tenure is expected for at least two reasons: first, because older and more senior workers are more likely to have been matched to (larger) and higher paying firms (Haltiwanger et al., 2018); and, second, because their fixed effects are likely to be less subject to measurement error as they can be observed over a longer time span, thereby reducing the bias due to finite sampling and limited mobility. More educated workers are expected to be more efficient searchers who match to higher paying firms, while the indication that the correlation is small for college graduates may simply reflect the fact that college graduates in the Portuguese labor market are young and short-tenured. For its part, binding minimum wage legislation renders wages incapable of functioning as prices, preempting any possibility of discriminating among the market values of worker skills. In this sense, it is not surprising that the correlation between worker and firm fixed effects is strongly negative. But one should nevertheless recall that minimum wage earners tend to be at the beginning of their working careers, often working in low paying, small firms. With respect to gender, the lower degree of wage sorting observed among women may be related to the finding that women more frequently work in firms with monopsony power (Félix and Portugal, 2016). The measure of wage sorting is also strongly influenced by firm size. As before, this may reflect *true* wage sorting, in the sense that larger, more mature firms have more structured internal labor markets with job ladders that are more efficient in allocating higher skilled workers to better paying jobs – or, to some degree, this may simply reflect the fact that the firm fixed effect is determined with more precision whenever firms are larger and older. Within industries, it can be seen that Manufacturing and Other Services exhibit the highest correlation coefficients, in part because these are the industries with the largest average firm sizes. A similar argument can be made with respect to the Lisboa and North regions. The bottom line therefore is that compositional factors play a role in explaining wage sorting (for a discussion of wage sorting by compositional trends in education, age, and gender, see Bagger et al., 2013).

Table 7 around here

Table 7 investigates the sensitivity of wage sorting measures to different sampling plans. In this endeavor, we first compute, for each subsample, the linear correlation coefficients obtained from the estimated worker and firm fixed effects generated in the full sample regression (given in Table 1), which are by construction less prone to measurement error and, therefore, to limited mobility bias (see the first column of Table 7). Secondly, we estimate a new regression for each subsample (and corresponding largest connected set) and extract the (less precise) fixed effects to compute the correlation between the worker fixed effect and the firm fixed effect.

From the full sample we identify the four largest regions, four consecutive time intervals, and three groups of workers determined by their frequency in the sample. In each case correlations between the worker and firm fixed effects for the various categories from the full sample are compared with their counterparts from the subsample. In all cases, the correlations based on the full sample are larger than those for the subsamples, suggesting that the time span over which the worker is observed severely biases the correlations. This suggests that finite sample bias in the estimation of the fixed effects (the incidental parameter problem) as well as limited mobility bias are very important. It is patently clear that when we estimate the fixed effects from smaller, shorter lived samples we increase the likelihood of small or even negative correlations. Note also that the correlations decrease sharply in the last two periods and especially the most recent interval (2007-2013). The evolution of wage sorting is perhaps better depicted in Figure 1, where it is shown that the correlation between the worker and the firm fixed effect steadily declined from 0.308 in 1991 to 0.081 in 2013. This evolution is in sharp contrast with the evidence presented for Germany by Card et al. (2013) and for Denmark by Bagger et al. (2013), both of which studies point to a sharp increase in wage sorting over time.

A number of labor market trends may have contributed to the decrease of wage sorting over the last 20 years of the survey. First, the average size of Portuguese firms has declined noticeably (Braguinsky et al., 2011); second, the fraction of minimum wage earners has increased (Carneiro et al., 2014); third, the proportion of employed women has increased; and, finally, the fraction of college graduates has increased markedly. However, if we take each of the compositional changes, there is no indication that the reported trends lie behind the evolution of the correlation coefficients. Indeed, it is apparent that the decrease of wage sorting is observed within each category. In particular, the trend towards less positive wage sorting occurs primarily among high wage workers, as is shown in panels (a) and (b) of Figure 2 which compare the bivariate density function of worker and firm fixed effects for 1991 and 2013, respectively. As can be seen, the quadrant of the figure corresponding to high wage workers and high wage firms is thinning through time.

Figures 1 and 2 around here

7. What Can We Learn about Assortative Matching from the Estimation of Production Functions

There is a general consensus that good workers (i.e. the more productive ones) tend to earn higher wages. Therefore, it is possible to rank workers' productivity based on the individual permanent component of their wages, namely the worker fixed effects estimated from wage equations. Similarly, good firms (i.e. more productive ones) tend to have higher profits. However, these firms may pay lower or higher wages due to the presence of nonmonotonicities in the wage schedule. Indeed, high-productivity firms have better outside options than their low-productivity counterparts, which may exert downward pressure on their workers' wages. This can be particularly relevant for low-skilled workers who may end up being paid less than if they were working for less productive firms (Lopes de Melo, 2008). Non-monotonicities in the wage schedule also mean that wages reflect the marginal contribution to the value that the firm generates and it can be either the more productive or the less productive firms that derive higher marginal benefit from employing a better worker (Eeckhout and Kircher, 2011). Wages do not therefore necessarily increase with firms' productivity. As a result, simply ranking firms according to the wages they pay will not identify the most productive ones. Minimally, without additional data on the productivity of firms, it will not be possible to determine whether assortative sorting is positive or negative.

To test the hypothesis of assortative matching we rely on the estimation of a firm-specific measure of productivity that is filtered from the heterogeneity of job titles. Our approach can be viewed as a way of attenuating the attribution problem flagged by Eeckhout and Kircher (2011, 900). For each production function specification, our interest will focus on the estimate of the firm fixed effect which we take as our measure of productivity. Beginning with real sales as the output measure, we otain a raw firm productivity estimate purged of time effects from:

$$\ln[Q_{ft}/L_{ft}] = \mu_f + \delta_t + \varepsilon_{ft} \ , \tag{7a}$$

where Q_{ft} denotes real sales by firm f in year t, L_{ft} corresponds to the number of workers, μ_f identifies the firm fixed effect, δ_t represents year fixed effects, and ε_{ft} is an idiosyncratic random error term.

To account for the heterogeneity of labor inputs, we next include a job title fixed effect, as follows:

$$\ln[Q_{ft}/L_{ft}] = \mu_f + \eta_j + \delta_t + \varepsilon_{fjt} \ , \tag{7b}$$

where η_j identifies the job-title fixed effect.

Alternatively, we can employ a much-reduced set of occupation fixed effects:

$$\ln[Q_{ft}/L_{ft}] = \mu_f + \gamma_o + \delta_t + \varepsilon_{fot} \ , \tag{7c}$$

where γ_o identifies the occupation fixed effect.

In our final specification with sales as the output variable, we estimate the parameters of a (Cobb-Douglas type) production function, where, for each firm, we consider as many potential labor inputs as there are occupations:

$$\ln Q_{ft} = \mu_f + \sum_{o \in O_{ft}} \omega_o ln L_{fot} + \delta_t + \varepsilon_{ft} \ , \tag{7d}$$

where O_{ft} is the set of existing occupations in firm f at year t, L_{fot} gives the number of workers filling occupation o at firm f in year t, and ω_o are output elasticities with respect to occupation o. The main reason we specify this production function is, as in Douglas (1976), to infer the size of the labor contribution to output. Since the total number of (harmonized) occupations is manageable ($n = 447$) this equation can be estimated using conventional fixed effects regression estimators.

The estimation results corresponding to these four specifications are given in the first four columns of Table 8. The correlation coefficients between the firm productivities obtained from the production function and the worker fixed effects extracted from the (three-way high dimensional fixed effects) wage regression are strikingly virtually identical, varying between 0.23 and 0.25. This suggests the presence of positive assortative matching and that the association between worker quality and the productivity of the firm is not significantly affected by labor input heterogeneity. Further, when we look at the pattern of these correlation coefficients over time, we observe a weakening of the association that closely mimics that earlier observed in the case of wage sorting (see Figure

1). This suggests that the factors underlying the decrease in wage sorting may also lie behind the decline in assortative matching. The correlation between the firm fixed effect from the wage equation and the firm fixed effect from the production function is large (around 0.46) but nevertheless considerably below 1, indicating that the mapping between a firm's wages and a firm's productivity is far from flawless.[10]

Table 8 around here

We now repeat the above exercise, using firm-level data on value added, thereby avoiding the issues of intermediate inputs while accounting for capital. We were able to merge a subset of firms (233,982) for the period 2006-2013 with the *Central de Balanços* dataset that provides extensive information on national accounting aggregates, including value added and capital. This procedure allowed us to estimate a standard Cobb-Douglas production function of type:

$$\ln Y_{ft} = \mu_f + \alpha \ln K_{ft} + \beta \ln L_{ft} + \delta_t + \varepsilon_{ft} \tag{7e}$$

where Y denotes value added, K stands for capital, and L measures labor input.

The results from the estimation of this equation are given in the sixth column of Table 8 (the raw correlation is provided in the fifth column). When we correlate our measure of total factor productivity, obtained from equation (7e), with the worker fixed effects we obtain a value of 0.23. Furthermore, when we expand this equation to include a measure of (the log of) the number of workers in a given occupation the correlation remains at 0.23 (column 7). And when we control for job title heterogeneity via the presence of job title fixed effects the correlation is somewhat reduced – to 0.18, see column 8 – suggesting that input

[10] Firm size and firm age play an altogether more muted role when we consider the correlation between the worker (wage) fixed effect and the firm fixed effect from the production function. This indicates that measurement error is likely to play a more important role in the AKM framework because it induces negative correlation between the worker and firm fixed effects. In terms of industries, however, there are no significant differences between the two approaches.

heterogeneity may play some role after all.[11]

Finally, we consider a model with as many potential labor inputs as there are job titles:

$$\ln Y_{ft} = \mu_f + \sum_{j \in J_{ft}} \xi_j lnL_{fjt} + \delta_t + \varepsilon_{ft} \ , \tag{7f}$$

where J_{ft} is the set of existing job-titles at firm f in year t, L_{fjt} denotes the number of workers, and ξ_j corresponds to the elasticity of output with respect to the labor for job title j.

Estimation of this multifactor production function is complicated by the sheer size of the matrix of covariates. For the 2006-2013 period for which we have value-added data available, we would need to obtain estimates for over 56,000 coefficients for the job-title elasticities, plus the year dummy coefficients and the critical firm fixed effects. [12] To proceed, therefore, we extended the iterative procedure of Guimarães and Portugal (2010) to this particular problem, by portioning the total set of covariates into smaller, manageable subsets (the approach is detailed in the Appendix). Although successful, the procedure converges very slowly and requires large storage requirements. Thus, to make the problem simpler, we restricted our data on manufacturing alone, which sector better fits the notion of a production function. With this restriction the number of job titles was downsized to around 33,000.

As before, the burden of this exercise is summarized in just one correlation coefficient. The number that we care about is the (employment-weighted) correlation coefficient between the firm averaged worker fixed effects extracted from the main wage equation (equation 1) and the firm fixed effects obtained from the production function. This correlation is 0.09, as can be seen from column

[11] Our results are broadly consistent with those given in Card et al. (2018) who show a positive impact of the mean of log value added per worker on estimated person effects, using Portuguese data. In their set-up, the empirical results are interpreted as indicating a sharing of economic rents whereas we place the emphasis on positive assortative matching.

[12] *Stata 15*, which we used to accomplish all estimations, has a hard-coded limit of 10,998 covariates.

9 of Table 8. We interpret this result as additional evidence that more productive workers tend to match with more productive firms. Overall, we have found robust empirical support for the notion of positive assortative matching.

These results can be seen as a useful generalisation of Mendes et al. (2010). Our measure of worker productivity, estimated from a three fixed effects wage equation that controls in particular for the heterogeneity of the firm's wage policies and the skill composition of its labor force, is better suited and more precise than the measure of workforce quality employed by these authors (namely, the proportion of hours worked by high-skilled workers in a firm as a share of total hours worked in that firm). Our measure of worker productivity is in turn correlated with alternative measures of firm-specific productivity that can also be estimated with great precision with the data at our disposal. In the case of equation (7f), one can think of our firm (productivity) fixed effects as a good proxy for firm total factor productivity; and one that takes into account the possible use of thousands of different labor inputs.[13]

The estimation results convey a consistent story in favour of the supermodularity or positive assortative matching hypothesis. The similarity between the magnitude of the wage sorting and the assortative matching correlations and, revealingly, their evolution over time seem to suggest that the correlations between the estimates of person and firm fixed effects from the wage equation may after all provide a sensible approximation to the measure of assortative matching. Contrary evidence from similar studies may be tied to the short temporal dimension of the panels used. As argued earlier, fixed sample and limited mobility biases seem the likely culprits here.

[13]Note that Mendes et al. (2010) estimate a panel regression in which the dependent variable is the log of real sales per hour worked in firm f in year t (as in our case) and where the independent variables are the logs of three time-varying worker quality indicators (*viz.* three skill categories, measured in terms of their contributions to total hours worked), their interactions, and two additional controls (the size of the workforce and an indicator for single-establishment firms). The specification chosen was a translog approximation for a generalized production function.

8. Conclusion

We have used a large longitudinal matched employer-employee dataset to estimate a wage equation with worker, firm, and job title fixed effects, having explored an econometric technique that provides an exact solution to the least squares estimation problem arising when estimating these three high-dimensional effects simultaneously. We decomposed the (natural log of) real hourly wages into five components: observed time-varying characteristics, worker heterogeneity (to include permanent observed and unobserved characteristics), firm heterogeneity (again both observed and unobserved), job title heterogeneity (idem), and a residual component.

We have reported that worker heterogeneity is the most important source of wage variation in Portugal (contributing to one third of the wage variation). The unobserved component plays a slightly less important role (16.4 percent) than the observed non-time varying characteristics of workers such as gender and education (16.6 percent). Firm effects were also found to be important (contributing one fourth), due in roughly equal parts to the unobserved component (12.6 percent) and to observed non-time-varying characteristics such regional location, capital ownership, and industry (12.0 percent). For their part, although less important than either of the corresponding worker or firm effects, job title fixed effects still explain an important one fifth of wage variation. This is comparable to the importance of the education in the standard Mincerian earnings function. The importance of job title effects in this treatment is that they are largely observed, having a basis in real world occupational diversity (implying compensating differentials and differential training needs stemming from complexity of tasks) and collective agreement impact. Note that job title effects serve to narrow the effect of unobserved worker heterogeneity, even if leaving its overall primacy unchallenged.

We have also reported that high-wage workers tend to be matched to firms paying higher wages, or "high-wage" firms. We first examined this wage sorting by different worker/firm categories and also by region and industry, and offered explanations for the pattern of findings (such as the lower degree of wage sorting observed for women than men). More important, however, was our investigation of the sensitivity of wage sorting to different sampling plans, the backdrop to which was our finding that, in marked contrast with much of the previous evidence, the connection between firms' compensation policies and the quality of their employees is positive. Our result is driven by the fact that we are largely attenuating finite sampling and limited mobility biases because of our use of the entire population of Portuguese wage earners in the private sector stretching over a long period of time. Our experiments confirmed that the main driver of these biases is the time span over which a worker is observed; that is to say, when we estimate the fixed effects from smaller, shorter lived samples we increase the likelihood of small or even negative correlations. Further contrasts with the wage sorting literature were apparent when we examined the time path of the correlations. Specifically, the correlation between the worker and the firm fixed effect obtained from the wage equation declined steadily from 0.308 in 1991 to 0.081 in 2013.

This brings us to the key, assortative matching component of the present treatment. As discussed in section 2, the generosity of firms' wage policies, as indexed by firm (wage) fixed effects cannot be taken as evidence that they are more productive. For this reason, we estimated firm-specific measures of productivity extracted from production functions, while carefully controlling for the heterogeneous composition of the workforce. Thus, while all regressions included firm fixed effects (firm productivity) and year fixed effects, some included job title fixed effects (or, in their place, job title elasticities) while others deployed occupation fixed effects (or, in their place, occupation elasticities). Separate output measure sales and value added per worker were employed. The sales functions could be estimated for the full sample period 1986-2013, while the value added functions could only be estimated for the period 2006-2013 using a different dataset.

Irrespective of the choice of output method, the specification of the production function, and the manner in which labor input heterogeneity was accounted for, evidence of positive assortative matching was reported throughout. Interestingly the correlation between the worker (wage) fixed effects obtained earlier and the firm productivity effect from the production function was around 0.2. This is broadly similar to the wage sorting correlation and like it also declining through time. Although the evidence is strongly supportive of supermodularity/positive assortative matching therefore, we are left with the puzzle of why there has been a weakening of matching through time. One possibility, consistent with the material in section 6 of this paper, would be firm downsizing among the largest firms. Another more speculative but linked line of reasoning would be the outsourcing of the jobs of less qualified workers.

Finally, there is the issue of external validity. Specifically, is there something particularly idiosyncratic about Portuguese wage determination and its wage distribution that sets Portugal apart from other nations. By way of a rejoinder, we would note that the sharp divergence between union density and union coverage is not confined to Portugal, whose collective bargaining system is not dissimilar from those in Spain, Italy, and France. Further, extension agreements (together with informal "orientation processes") are indeed common to many other continental European nations. Accordingly, while not denying the need for more work on the wage distributions of different countries, we consider it unlikely that institutional idiosyncrasies lie at the heart of the findings on worker and firm heterogeneity and assortative matching reported here for Portugal.

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Appendix: Estimation of the High-Dimensional Cobb-Douglas function

The Cobb-Douglas functions that were estimated in section 7 require the implementation of a high-dimensional linear regression with thousands of covariates and one fixed effect. With conventional software it is impossible to estimate these regressions. However these models can be estimated using a variant of the algorithm presented in Guimarães and Portugal (2010). To illustrate, consider the general linear regression model given by:

$$\mathbf{Y} = \mathbf{X}_1\beta_1 + \mathbf{X}_2\beta_2 + ... + \mathbf{X}_k\beta_k + \varepsilon$$

where for simplicity we are partitioning the total set of covariates into smaller subsets that have a dimension with is amenable for estimation with conventional software. Now, to estimate this model we can employ a strategy which is similar to the one employed for estimation of our wage equation. Rewriting the normal equations as:

$$\begin{bmatrix} (\mathbf{X}_1'\mathbf{X}_1)\beta_1 \\ (\mathbf{X}_2'\mathbf{X}_2)\beta_2 \\ ... \\ (\mathbf{X}_k'\mathbf{X}_k)\beta_k \end{bmatrix} = \begin{bmatrix} \mathbf{X}_1'(\mathbf{Y} - \mathbf{X}_2\beta_2 - \mathbf{X}_3\beta_3 - ... - \mathbf{X}_k\beta_k) \\ \mathbf{X}_2'(\mathbf{Y} - \mathbf{X}_1\beta_1 - \mathbf{X}_3\beta_3 - ... - \mathbf{X}_k\beta_k) \\ ... \\ \mathbf{X}_k'(\mathbf{Y} - \mathbf{X}_1\beta_1 - \mathbf{X}_2\beta_2 - ... - \mathbf{X}_{k-1}\beta_{k-1}) \end{bmatrix} = \begin{bmatrix} \mathbf{X}_1'\mathbf{Y}_1^* \\ \mathbf{X}_2'\mathbf{Y}_2^* \\ ... \\ \mathbf{X}_k'\mathbf{Y}_k^* \end{bmatrix}$$

we can iterate across the equations taking as known the βs that show up on the right-hand side (replacing them by the last known estimates) and estimating the βs on the left-hand side by implementing the linear regression implied by the above equations. Again, this is a process that will converge slowly but steadily. A fixed effect can be easily added. One option is to assume that one of the subsets consists of all the dummy variables that define the fixed effect. In this case the estimation step associated with the fixed effect consists on the calculation of simple group means.

Table 1: Fitted wage equation with worker, firm, and job title fixed effects

Variable	Two HD fixed effects	Three HD fixed effects
Age squared (years)	-0.0003	-0.0002
	$(-5,069.7)$	$(-5,724.4)$
Tenure	0.0095	0.0068
	(847.2)	(541.9)
Tenure squared	-0.0020	-0.0016
	(-558.5)	(-393.0)
Firm size (in logs)	0.0426	0.0431
	$(2,645.8)$	$(4,013.1)$
Year Dummies	Yes	Yes
Worker fixed effects	Yes	Yes
Firm fixed effects	Yes	Yes
Job-title fixed effects	No	Yes
Number of observations	$36,558,896$	$36,558,896$
R-squared	0.8801	0.8915

Note: t-statistics (in parentheses) are calculated with worker-cluster robust standard errors.

Table 2: Regression estimates of worker fixed effects on non-time-varying worker characteristics

Variable	Coefficient
Female	-0.1428
	(-668.7)
Schooling	
First stage of basic education	0.0768
	(167.6)
Second stage of basic education	0.1516
	(304.6)
Third stage of basic education	0.2164
	(414.3)
Secondary education	0.2825
	(517.5)
Post secondary education	0.4023
	(225.9)
First stage of tertiary education	0.5070
	(499.4)
Second stage of tertiary education	0.5425
	(682.6)
Birth year dummies	Yes
Observations	$36,558,896$
R-squared	0.6440

Note: t-statistics (in parentheses) are calculated with worker-cluster robust standard errors.

Table 3: Regression estimates of firm fixed effects on non-time varying firm characteristics

Variable	Coefficient
Regions	
Center	0.0178
	(5.4)
Lisboa and Tagus Valley	0.0802
	(15.9)
Alentejo	0.0502
	(8.0)
Algarve	0.1039
	(19.6)
Madeira and Azores	0.0796
	(14.0)
Share of domestic capital	-0.14
	(-10.9)
Share of public capital	0.03
	(0.8)
Industry dummies	Yes
Observations	639,708
R-squared	0.3235

Note: Regression weighted by firm employment. t-statistics (in parentheses) are calculated with firm-cluster robust standard errors.

Table 4: Correlations between compensation components (Three HD fixed effects)

		1	2	2.1	2.2	2.3	3	3.1	3.2	4	4.1	4.2	5	5.1	5.2
log of real hourly wage (1986 prices)	1	1													
Predicted effects of X variables[a]	2	0.231	1												
Time	2.1	0.234	0.810	1											
Worker time-varying	2.2	−0.192	0.384	−0.134	1										
Firm time-varying	2.3	0.369	0.171	−0.135	−0.034	1									
Worker fixed effects	3	0.535	−0.533	−0.344	−0.537	0.200	1								
Unobserved component	3.1	0.431	−0.009	−0.063	0.053	0.070	0.600	1							
observed component[b]	3.2	0.346	−0.658	−0.382	−0.707	0.198	0.803	0.000	1						
Firm fixed effects	4	0.613	0.042	−0.026	−0.050	0.291	0.196	0.018	0.230	1					
unobserved component	4.1	0.380	−0.001	−0.013	−0.012	0.053	0.051	−0.049	0.100	0.823	1				
observed component[c]	4.2	0.531	0.083	−0.014	−0.076	0.432	0.271	0.106	0.259	0.569	0.000	1			
Job title fixed effects	5	0.670	0.046	0.090	−0.151	0.133	0.413	0.201	0.365	0.256	0.086	0.330	1		
unobserved component	5.1	0.260	−0.066	0.015	−0.115	0.023	0.205	0.084	0.193	0.023	0.023	0.014	0.551	1	
observed component[d]	5.2	0.622	0.084	0.090	−0.080	0.137	0.352	0.188	0.298	0.286	0.089	0.382	0.834	0.00	1

[a] The time-varying observable characteristics of workers (firms) are age squared, seniority, and seniority squared (firm size). [b] Gender and seven education dummies. [c] Capital ownership (shares of domestic and public capital), five region dummies, and twenty-eight industry dummies. [d] Occupation and collective agreement.

Table 5: Contributions of compensation components to wage variation

		Two HD fixed effects	Three HD fixed effects
Total	1	**100.0**%	**100.0**%
Predicted effects of X variables[a]	2	13.0%	12.4%
Time	2.1	14.8%	11.0%
Worker time-varying	2.2	−7.9%	−4.9%
Firm time-varying	2.3	6.1%	6.2%
Worker fixed effects	3	47.9%	33.0%
unobserved component	3.1	21.2%	16.4%
observed component[b]	3.2	26.7%	16.6%
Firm fixed effects	4	27.1%	24.6%
unobserved component	4.1	13.3%	12.6%
observed component[c]	4.2	13.8%	12.0%
Job title fixed effects	5		19%
unobserved component	5.1		4.1%
observed component [d]	5.2		14.9%
Residual	6	12.0%	11.0%

[a] The time-varying observable characteristics of workers (firms) are age squared, seniority, and seniority squared (firm size). [b] Gender and seven education dummies. [c] Capital ownership (shares of domestic and public capital), five region dummies, and twenty-eight industry dummies. [d] Occupation and collective agreement.

Table 6: Correlation between worker and firm fixed effects by worker and firm characteristics

Age		Firm Size	
16-25	-0.031	1st quartile (<= 13)	-0.104
26-45	0.188	2d quartile (14-56)	0.136
> 45	0.253	3d quartile (57-299)	0.208
		4th quartile (> 299)	0.324
Gender		Firm Age	
male	0.214	1st quartile (<= 9)	0.177
female	0.177	2d quartile (10-17)	0.151
		3rd quartile (18-30)	0.168
Wages		4th quartile (>= 31)	0.255
minimum wage earner	-0.371	Industries	
non-minimum wage earner	0.183	Mining	0.136
		Manufacturing	0.218
Tenure		Utilities	-0.080
<= 5	0.076	Construction	0.049
6-20	0.222	Trade	0.099
> 20	0.309	Hotels and Restaurants	0.004
		Transportation	0.005
Education		Other Services	0.279
No schooling	0.126	Region	
Basic I	0.169	North	0.132
Basic II	0.194	Center	0.009
Basic III	0.241	Lisboa and Tagus Valley	0.233
Secondary	0.219	Alentejo	0.022
Tertiary	0.126	Algarve	-0.052
		Madeira and Azores	0.106

Table 7: Correlation between worker and firm fixed effects for selected subsamples

	overall regression	regression on subsamples
District		
Lisboa ($N = 11,828,134$)	0.241	0.220
Porto ($N = 7,340,920$)	0.194	0.141
Braga ($N = 3,400,304$)	-0.016	-0.071
Aveiro ($N = 2,771,477$)	0.026	0.007
Period		
1986-1993 ($N = 6,838,724$)	0.282	0.051
1994-2000 ($N = 8,722,171$)	0.273	0.086
2002-2007 ($N = 8,331,974$)	0.163	0.000
2007-2013 ($N = 8,061,445$)	0.103	-0.032
Number of Years in the Sample		
$<= 5$ ($N = 7,232,389$)	0.122	-0.052
$6 - 15$ ($N = 19,243,021$)	0.245	0.164
$>= 16$ ($N = 8,819,214$)	0.362	0.134

Table 8: Correlations between firm fixed effects obtained from production functions and worker and firm fixed effects from the wage equation

	Sales pw	Sales pw	Sales pw	Sales	VA pw	VA	VA	VA pw	VA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Job title elasticities	No	No	No	No	No	No	No	No	Yes
Job title fixed effects	No	Yes	No	No	No	No	No	Yes	No
Occupation elasticities	No	No	No	Yes	No	No	Yes	No	No
Occupation fixed effects	No	No	Yes	No	No	No	No	No	No
Labor	No	No	No	Yes	No	Yes	Yes	No	Yes
Capital	No	No	No	No	No	Yes	No	Yes	No
Corr with worker fe	0.2390	0.2377	0.2335	0.2479	0.2548	0.2347	0.2292	0.1763	0.0919
Corr with firm fe	0.4673	0.4659	0.4655	0.5133	0.5455	0.5315	0.5345	0.4433	0.2429
Sample period	1986/2013	1986/2013	1986/2013	1986/2013	2006/2013	2006/2013	2006/2013	2006/2013	2006/2013
Number of firms	599,485	599,485	519,862	599,555	233,982	222,788	243,562	199,953	42,041
Number of observations	34,228,554	34,228,554	32,175,460	32,175,460	24,843,831	24,639,123	24,363,666	24,146,663	9,350,135

Note: All production functions include firm and year fixed effects. With the exception of specifications (4) and (9) all regressions are weighted by the number of workers in each cell of the corresponding equation. Labor is measured as the log of number of workers and Capital is the log value of total physical assets.

Table A1: Descriptive statistics ($N = 36,558,896$)

	Mean	S.D.
Real hourly wages (in logs)	0.3357	0.5683
Age (in years)	37.4732	10.9332
Tenure (in years)	8.3221	8.4883
Gender (female=1)	0.4150	–
Schooling		
No Schooling	0.0241	–
First stage of basic education	0.3216	–
Second stage of basic education	0.2129	–
Third stage of basic education	0.1865	–
Secondary education	0.1765	–
Post secondary education	0.0026	–
First stage of tertiary education	0.0163	–
Second stage of tertiary education	0.0595	–
Regions		
North	0.3644	–
Center	0.1787	–
Lisboa and Tagus Valley	0.3523	–
Alentejo	0.0413	–
Algarve	0.0311	–
Madeira and Azores	0.0321	–
Firm size (in logs)	4.2877	2.2104
Share of domestic capital	0.9024	0.2815
Share of public capital	0.0480	0.2071
Number of jobs per worker	1.794	1.1817
Fraction of movers	0.4309	-

Table A2: Correlations between compensation components (Two HD fixed effects)

		1	2	2.1	2.2	2.3	3	3.1	3.2	4	4.1	4.2
log of real hourly wage (1986 prices)	1	1										
Predicted effects of X variables[a]	2	0.188	1									
Time	2.1	0.239	0.788	1								
Worker time-varying	2.2	−0.1942	0.459	−0.133	1							
Firm time-varying	2.3	0.370	0.107	−0.124	−0.035	1						
Worker fixed effects	3	0.553	−0.594	−0.348	−0.562	0.201	1					
Unobserved component	3.1	0.459	−0.014	−0.070	0.058	0.063	0.533	1				
observed component[b]	3.2	0.364	−0.693	−0.367	−0.701	0.198	0.847	0.000	1			
Firm fixed effects	4	0.639	0.024	−0.013	−0.058	0.288	0.229	0.025	0.255	1		
unobserved component	4.1	0.392	−0.004	−0.008	−0.008	0.034	0.065	−0.052	0.109	0.800	1	
observed component[c]	4.2	0.541	0.044	−0.011	−0.085	0.435	0.295	0.110	0.280	0.600	0.000	1

[a] The time-varying observable characteristics of workers (firms) are age squared, seniority, and seniority squared (firm size). [b] Gender and seven education dummies. [c] Capital ownership (shares of domestic and public capital), five region dummies, and twenty-eight industry dummies. [d] Occupation and collective agreement.

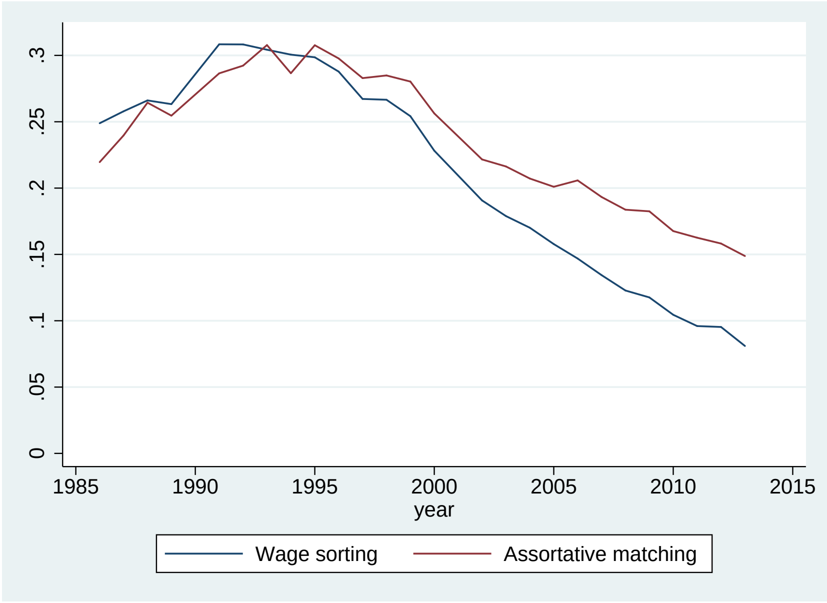
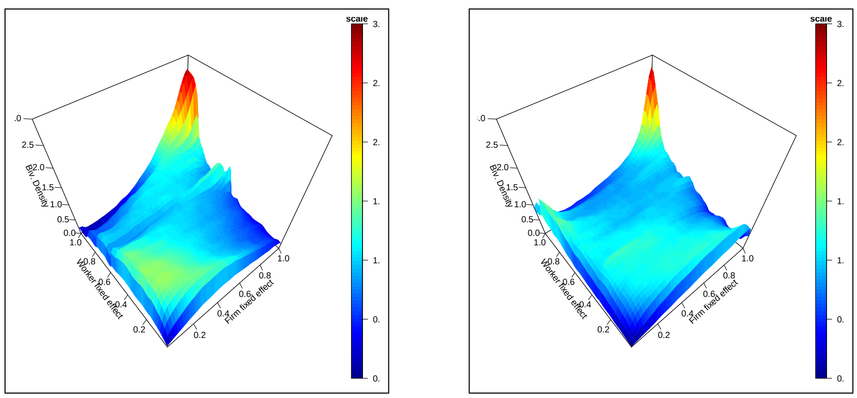


Figure 1: Correlation between the worker and the firm fixed effect over time



(a) Panel A (1991)

(b) Panel B (2013)

Figure 2: Bivariate density of worker and firm fixed effects